(3)[3] $\log E_C = \log E_\sigma - \log A$ [since $E_C = E_\sigma \div A$].

Now, if one is working with a calculating machine, he can simply carry the value of E_A to two or three more decimal places than are to be retained, and then divide by the square root of 2 to get E_σ; similarly, the latter value, divided by the mean, gives E_C.

The writer prefers, however, to calculate the values in the ordinary way on the machine, using Miss Gibson's[4] table for $\frac{.6745}{\sqrt{n}}$ and $\frac{.6745}{\sqrt{2n}}$, and then to use the short method in checking. The original computations can be indicated and performed with great confidence and rapidity, since it is hardly possible to make an error that will not be discovered in the checking.[5] It is obviously safer, as well as much quicker, to check in this way than to repeat the original processes.

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GALTON AND DISCONTINUITY IN VARIATION

It seems not to be generally realized that Galton recognized both continuity and discontinuity, both in variation and inheritance. Of course, all biologists are familiar with "Galton's polygon," in which slight oscillations of the polygon on one of its faces, but without a change of face, are compared with "small unstable deviations" (fluctuations), while a larger oscillation, in which the polygon moves over to a new face, is compared to a

> sport . . . of such marked peculiarity and stability as to rank as a new type, capable of becoming the origin of a new race with very little assistance on the part of natural selection.[1]

Galton's polygon illustrated for him how the following conditions may co-exist:

> (1) Variability within narrow limits without prejudice to the purity of the breed. (2) Partly stable sub-types. (3) Tendency, when much disturbed, to revert from a sub-type to an earlier form. (4) Occasional sports which may give rise to new types.

These four types would seem to correspond rather well to what

[3] Formula (3) gives, of course, the approximate or uncorrected value of E_C.

[4] Gibson, Winifred, "Tables for Facilitating the Computation of Probable Errors," *Biometrika*, 4: 385–393. 3 tables.

[5] Unless, of course, one misreads the figures from the machine in checking.

[1] "Natural Inheritance," London, 1889, p. 28.

are now called (1) fluctuations or "non-inherited" (in reality, I think, partially inherited) continuous variations; (2) instability resulting from a heterozygous or partially heterozygous condition; (3) reversions, now believed to result chiefly from crossing; and (4) mutations.

Galton is equally explicit in other statements on this subject. Like Darwin, he admitted the facts both of continuity and discontinuity in variation; but, unlike Darwin, he also recognized discontinuity or alternation as well as continuity or blending, in inheritance. Thus he says, in a paragraph headed "stability of sports":[2]

Experience does not show that those wide varieties which are called "sports" are unstable. On the contrary, they are often transmitted to successive generations with curious persistence. Neither is there any reason for expecting otherwise. While we can well understand that a strained modification of a type would not be so stable as one that approximates more nearly to the typical center, the variety may be so wide that it falls into different conditions of stability, and ceases to be a strained modification of the original type.

In another paragraph,[3] headed "Evolution not by minute steps only," he says:

The theory of evolution might dispense with a restriction, for which it is difficult to see either the need or the justification, namely, that the course of evolution always proceeds by steps that are severally minute, and that become effective only through accumulation. That the steps *may* be small and that they *must* be small are very different views; it is only to the latter that I object. . . . An apparent ground for the common belief is founded on the fact that wherever search is made for intermediate forms between widely divergent varieties, whether they be of plants or of animals, of weapons or utensils, of customs, religion or language, or of any other product of evolution, a long and orderly series can usually be made out, each member of which differs in an almost imperceptible degree from the adjacent specimens. But it does not at all follow because these intermediate forms have been found to exist, that they are the very stages that were passed through in the course of evolution. Counter evidence exists in abundance, not only of the appearance of considerable sports, but of their remarkable stability in hereditary transmission.

Again, Galton not only believed in the existence of both blended and alternative inheritance, but he recognized the im-

[2] *L. c.*, p. 30.
[3] *L. c.*, p. 32.

portance of the latter in connection with the survival of new races. Thus he writes:[4]

> The quadroon child of the mulatto and the white has a quarter tint; some of the children may be altogether darker or lighter than the rest, but they are not piebald.[5] Skin-color is therefore a good example of what I call blended inheritance. . . .
>
> Next as regards heritages that come altogether from one progenitor to the exclusion of the rest. Eye-color is a fairly good illustration of this. . . .
>
> There are probably no heritages that perfectly blend or that absolutely exclude one another, but all heritages have a tendency in one or the other direction, and the tendency is often a very strong one.

On the following page Galton remarks that

> A peculiar interest attaches itself to mutually exclusive heritages, owing to the aid they must afford to the establishment of incipient races.

He thus recognizes the invalidity of Darwin's objection to "single variations" as a factor in evolution, namely, that they would certainly be swamped by crossing with the general population.

It would, therefore, appear that in his recognition of continuity as well as discontinuity both in variation and heredity, Galton was in advance of his time, and more in accord with some of the current views.

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REPULSION IN MICE

IN the February number of the AMERICAN NATURALIST Dr. C. Little criticizes the results of my mouse-breeding experiments which I published in the *Zeitschrift für Induktive Abstammungs- und Vererbungs-lehre* Bd. VI, Heft 3. The chief point, on which he disagrees with me, is the interpretation of the results I obtained in breeding black and albino mice together.

The fact is, that in my paper on mice, I overlooked a serious error. In three sentences on page 126, relating to test matings of albinos, the words "black" and "agouti" changed places. As printed in the paper these sentences run:

> Without exception they have given *black* or equal numbers of *black* and albino young, depending upon the purity of the black used. But never has one of these albinos produced a single *agouti* young in a mating with black. Counting together the colored young of such families I get 89 *black* young.

[4] *L. c.*, p. 12.

[5] Cases of piebaldism in such crosses are of course now well-known.